Community-Based Rangeland and Livestock Management

Pre-Analysis Plan

**Partners:** Millennium Challenge Corporation (funding), GOPA Consultants (implementation)

Purpose of Study

This document outlines the analysis plan for the project “Community-Rangeland and Livestock Management”. The primary academic purpose of the study is to test the impact of a community-based livestock and rangeland management program in northern Namibia on livestock assets, income, social cohesion and rangeland health.

It is theorized that pastoral farmers in communal areas like northern Namibia face a “tragedy of the commons” problem regarding the choice of where to graze and how many cattle to keep, and that formalizing community resource management practices (via group meetings, defined responsibilities and sanctions), as GOPA sought to do, can alleviate this problem.

GOPA also tried to introduce animal husbandry best practices, including herd restructuring to reduce the share of unproductive cattle and use of vaccinations and supplements. Similarly, GOPA sought to teach farmers to commercialize their practices and sell more of their animals through the formal market.

This evaluation also speaks to a body of literature in ecology on holistic rangeland management, the theory that guided GOPA’s farmer training. This technique, which involves grazing cattle in large combined herds and following a regular rotational plan, has been posited to improve the long-term health of the rangeland in areas at risk of desertification.

In addition to instituting community groups as well as regional livestock marketing cooperatives and training on animal husbandry and rangeland management, the CBRLM project provided direct support to farmers in the form of installing water points, providing matching funds to community groups and implementing a livestock pass-on scheme.

To address all these potential avenues of impact, this analysis will examine changes in farmer behaviors and attitudes, cattle herd structure and health, rangeland quality and grass availability, and household well-being.

Sample Size and Attrition

|  |  |  |
| --- | --- | --- |
| **Survey** | **Sample Size Targeted** | **Attrition** |
| Kraal Manager Survey (2014) | 1271 | 12 |
| Kraal Manager Survey (2016) | 1348 | 50 |
| Cattle Weighing Survey (2016) | 694 | 25 |
| Rangeland Survey - Rainy Season (2016) | 1230 | 2561 |
| Rangeland Survey - Dry Season (2016) | 10722 | 1301 |
| Household Survey (2017) | 1265 | 25 |

1. These numbers represent randomly placed rangeland sites that were either ineligible for data collection (due to) or inaccessible (due to). Since these “abandoned” sites should not have been part of our sampling frame to begin with, their exclusion should not introduce bias. Moreover, abandonments are balanced between treatment and control GAs.
2. Due to the large number of controlled forest burnings in the Kavango region, which rendered several previously accessible sites barren in the dry season, the decision was made to add 3 additional sites in each Kavango GA, for a total of 98 new sites in the second round of surveying. There is no observed correlation between site burning and treatment status or vegetation type.

Dependent Variables

We will examine outcomes in the following areas. A full list of variables in each index are available [here](https://docs.google.com/spreadsheets/d/1vwjwiWPiOZO1L94KljxdOMFmzP-szaVqaBPBzecKRzE/edit#gid=1661584276).

**1 - Takeup and Inputs**

* **Takeup (not indexed)**
	+ Was GOPA offered - individual
	+ Did you participate in GOPA - individual
	+ Was GOPA offered - community
	+ Did you participate in GOPA - community
	+ Was GOPA offered - TA
	+ Did you participate in GOPA - TA
* **Project Inputs (not indexed)**
	+ Water points - new
	+ Water points - fix
	+ Water points - spending
	+ Bulls
	+ Small-stock pass-on
	+ GA matching fund - binary
	+ GA matching fund - amount

**2 - Behaviors, Beliefs, and Community Dynamics**

* **Rangeland Management Behavior**
	+ Grazing plan - Planning (2014)
	+ Grazing plan - Planning (2016)
	+ Grazing plan - Adherence (2014)
	+ Grazing plan - Adherence (2016)
	+ Rangeland Management - Optimal Practices (2014)
	+ Rangeland Management - Optimal Practices (2016)
	+ Labor Management (2014)
	+ Labor Management (2016)
* **Livestock Management Behavior**
	+ Animal Husbandry / Livestock Management (2014)
	+ Animal Husbandry / Livestock Management ( 2016)
	+ Herd Structuring Practices (2014)
	+ Herd Structuring Practices (2016)
	+ Livestock Marketing Behaviors (2014)
	+ Livestock Marketing Behaviors (2016)
* **Community Dynamics**
	+ Collective Action (2014)
	+ Collective Action (2016)
	+ Community Governance (2014)
	+ Community Governance (2016)
	+ Conflict (2014)
	+ Conflict (2016)
	+ Trust (2014)
* **Knowledge, Attitudes, and Beliefs**
	+ Self and community efficacy (2014)
	+ Self and community efficacy (2017)
	+ Rangeland / Livestock Knolwedge (2016)

**3 - Material Outcomes**

* **Rangeland** (a full discussion of the logic of ecological change is [here](https://docs.google.com/document/d/1OhnGA6VxZnq44RICcHi5kxv2_xhjrV6TnRPzVZbiq5U/edit))
	+ Bare Ground
	+ Woody Vegetation
	+ Herbaceous Cover
	+ Grass / Forb Mix
	+ Perennial / Annual Mix
	+ Weeds
	+ Erosion
	+ Remote Sensing Vegetation Index (drought and non-drought years)
* **Cattle**
	+ Kraal Cattle Wealth
	+ Herd Structure
	+ Herd Productivity
* **Household Well-Being**
	+ Net Income (Directly calculated, not index)
		- Income (cattle and non-cattle)
		- Expenditures and consumption (cattle and non-cattle)
	+ Household Livestock Wealth
	+ Time Use
	+ Resilience
	+ Female Empowerment
	+ Diet

Each of the outcomes in lists 2 and 3 (the sub-bullets) is an index of component variables, except for the rangeland variables, which will be analyzed individually. We excluded variables that had a low observed variance (blinded to treatment assignment).

Indices will be constructed according to the following procedure:

1. Standardize each component variable against the control mean.
2. Calculate the index by taking unweighted average of the component variables.
3. Standardize the index against its control mean.

In the event that a component variable is missing for a given observation, it will be omitted from the calculation of the index. This is equivalent to imputing the value of the missing variable as the mean of the other component variables.

Randomization and Sample Selection

The unit of randomization in this study is the Rangeland Intervention Area (RIA). RIAs are essentially intervention zones that share a commonly agreed upon boundary and a common authority over what happens within the area. Those RIAs selected to be part of the treatment group received the package of CBRLM activities while those RIAs selected for the control group did not.

The 41 RIAs in our sample were randomly assigned to either Treatment or Control. For primarily political reasons, the RIAs were stratified on a single variable: affiliation with a Traditional Authority (TA). This was to ensure that at least half of every politically-sensitive TA was included in the CBRLM intervention. Treatment assignment was also balanced on the following RIA-level variables: Traditional Authority, vegetation type, number of households, number of cattle, cattle density, quality of water source, community based organizations, and overlap with complementary interventions.

**Revised Evaluation Design**

The original sampling strategy for data collection – i.e., the strategy that was followed at baseline – was ultimately deemed unviable due to insufficient overlap between the areas surveyed at baseline and the areas of program implementation.

The original sampling strategy was based on GOPA’s ex-ante expectations of where the organization would generally focus its early implementation efforts (i.e., the “green areas”). However, over the course of 2011 it became apparent that many of GOPA’s *actual* implementation efforts were happening outside of these pre-identified areas. MCC and MCA-Namibia helped convene a series of meetings in which IPA and GOPA used ArcGIS mapping technology to roughly estimate the level of take-up in “green areas” versus non-“green areas” within treatment RIAs. The key take-away from these meetings was that the upper bound for take-up in “green areas” was approximately 25%, which fell well short of the 70% take-up rate upon which the initial statistical power calculations had been based.

Starting in 2012 and ending in 2013, IPA researchers traveled to all 41 RIAs and worked with Traditional Authorities (TAs) and Agricultural Extension (AgExt) officers to define and map all the grazing areas in each region. IPA mapped 384 GAs in all - 171 in control areas and 213 in treatment areas.

IPA’s field team then collected information about each GA from interviews with TAs and AgExt officers, the Namibian census, and the Atlas of Namibia, a geo-referenced database on climate, ecology, and livestock in the country.[[1]](#footnote-1)

Using this data, IPA developed a statistical model that used the pre-program characteristics of GAs to predict CBRLM take-up. This model generated a “probability of takeup” for each GA between 0 and 1. IPA then set a cutoff point to filter “predicted” from “non-predicted” GAs. In treatment areas, 52 GAs were predicted, 37 of which were “active” CBRLM GAs and 15 of which were “non-performing”. In control areas, 71 GAs were above the “predicted” cutoff.

The resulting sample of 123 GAs was balanced between treatment and control on the characteristics in the model, as well as a variety of bio-physical characteristics like rainfall, livestock density, and forage cover (although these measures are quite rough) and individual-level variables like average farmer age, educational attainment, gender, and ethnic identity. Hence this step is critical for establishing the validity of the randomized trial and orthogonality of assignment of treatment to baseline variables, both behavioral and environmental/land.

Modeling

We will run and report the results of three different model specifications, reflecting different norms of the disciplines of the outcomes under study.

*Clustered Standard Errors*

The first model is a conservative single-level regression of treatment status on outcomes, with standard errors clustered at the RIA level.

*GLMM*

Second, we plan to use a generalized linear mixed model (GLMM), which is the typical approach in rangeland ecology for multilevel data. The “mixed” component accommodates clustering of observations and constructs hypothesis tests based on appropriate degrees of freedom; for example, a test of grazing intervention should be based on the number of RIAs, not the number of GAs or sites. The “generalized” component allows for response variables that follow distributions other than the normal; for example, a beta distribution or a fractional logistic model is likely to be more appropriate for analyses of percent vegetation cover. The validity of the “linear” component depends upon the nature of the relationships between response and continuous explanatory variables; nonlinear relationships may be accommodated using splines in the linear model framework.

For the GLMM, the observation conditional on the random effects y|b is distributed according to a distribution in the exponential family, with E(y|b) = u|b and Var(y|b) = R . The link function is h = g(u|b) and the linear predictor is h = XB + Zb , where b~*N*(0,G), X is the design matrix for the fixed effects (i.e., explanatory variables, or covariates), B is the vector of fixed effects parameters, Z is the design matrix for the random effects, and b is the vector of random effects parameters. RIAs, GAs nested within RIAs, and sites nested within GAs are incorporated as random effects factors. The choice of link function depends upon the assumed distribution of y|b.

*Randomization Inference*

Third and finally, we will test outcomes with a randomization inference procedure. We will re-run the randomization procedure (described above) 10,000times and generate an Average Treatment Effect under each randomization. These simulated randomizations generate an estimated distribution of of the ATE under the sharp null hypothesis of no treatment effect (Y(0) = Y(1)). We then calculate the probability of recovering an ATE at least as large as the one estimated in our experiment under the assumption of a sharp null. The p-value is the percent of re-randomizations that generate a treatment effect at least as large or larger than the true ATE.

Robustness Checks

*Multiple hypothesis adjustment*

We will apply a multiple hypothesis test correction within the following groups of outcomes:

* **Behavioral outcomes (2014):**
	+ Grazing plan - Planning
	+ Grazing plan - Adherence
	+ Rangeland Management - Optimal Practices
	+ Labor Management
* **Behavioral outcomes (2016):**
	+ Grazing plan - Planning
	+ Grazing plan - Adherence
	+ Rangeland Management - Optimal Practices
	+ Labor Management
* **Primary material outcomes:**
	+ Kraal Cattle Wealth
	+ Herd Productivity
	+ Net Income (Directly calculated, not index)
		- Income (cattle and non-cattle)
		- Expenditures and consumption (cattle and non-cattle)
	+ Household Livestock Wealth
* **Secondary material outcomes:**
	+ Time Use
	+ Resilience
	+ Female Empowerment
	+ Diet
	+ Herd Structure
* **Mechanisms:**
	+ Collective Action (2014, 2016)
	+ Community Governance (2014, 2016)
	+ Conflict (2014, 2016)
	+ Trust (2014)
	+ Self and community efficacy (2014, 2017)
	+ Rangeland / Livestock Knowledge (2016)

We will not apply any correction to the “first stage” outcomes (project takeup), the rangeland outcomes or the exploratory analyses.

We will follow the “false discovery rate” method of multiple hypothesis correction, per Hochberg (1988). We will report both corrected and uncorrected coefficients in our final results.

*Outlier adjustment and household size adjustment*

To decide how to deal with outlier issues, we will select the variable specification that has the greatest statistical precision in our blinded treatment estimates, following the procedure below:

1. Randomly assign a “dummy” treatment status.
2. Regress each version of the variable (i.e. adjusted for outliers or not, adjusted for household size or not) on the dummy treatment status.
3. Select the specification that has the lowest level of standard error divided by dependent variable standard deviation.
4. If we discover upon unblinding the data that household size is significantly affected by a treatment, then we will normalize by household size regardless of the standard errors on the dummy treatment effects.

Using this decision rule, we will consider two ways of dealing with outliers - winsorizing (i.e. replacing the top decile of outcomes with the 90th percentile value) or taking the logarithm of the variable. Additionally, we will consider adjusting household income and expenditure for the size of the household by dividing by the number of “adult equivalent” household members, defined as follows:

(A + αK)β

where A is the number of adults, K is the number of children, α is the cost of a child relative to an adult (here 0.3, the convention for developing countries) and β is a parameter between 0 and 1 (here 0.9, the convention for developing countries) that captures economies of scale.

*Attrition*

Attrition in the study was low (5% or less across all survey rounds):

|  |
| --- |
| **Attrition Rates** |
| **Behavioral Survey (2014)** |
| **Kunene** | **Cattle Post** | **Ohangwena** | **Kavango** |
| 1.58% | 1.15% | 0.85% | 0.00% |
| **Behavioral Survey (2016)** |
| **Kunene** | **Cattle Post** | **Ohangwena** | **Kavango** |
| 4.24% | 5.48% | 1.44% | 1.08% |
| **Treatment** | **Control** |  |  |
| 2.74% | 3.50% |  |  |
| **Cattle Survey (2016)** |
| **Kunene** | **Cattle Post** | **Ohangwena** | **Kavango** |
| 1.46% | 7.41% | 6.06% | 2.60% |
| **Treatment** | **Control** |  |  |
| 3.72% | 3.26% |  |  |
| **Household Survey (2017)** |
| **Kunene** | **Cattle Post** | **Ohangwena** | **Kavango** |
| 0.85% | 5.86% | 2.21% | 0.54% |
| **Treatment** | **Control** |  |  |
| 3.05% | 1.34% |  |  |

In general, attrition was driven by respondents who were either unavailable due to a life event (such as imprisonment or a death in the family) or who work in southern Namibia. It is unlikely that either of these things were affected by treatment to a meaningful degree. Very few instances of attrition were due to a refusal to speak to surveyors. However, the regional differences in attrition may affect regional heterogeneous treatment effects, so those results should be interpreted with caution.

Additionally, to test the robustness of our assumption that attrition is independent of potential outcomes, we apply two different methods of estimating upper and lower bounds on treatment effects. For continuous variables, we apply the trimming bounds procedure. This method only offers an estimate of bounds for subjects who would have responded regardless of their assignment to treatment or control (“always-reporters”). However, it still offers an important robustness check.

For binary variables, we apply the extreme value bounds procedure, in which upper and lower bounds on treatment effect are calculated by replacing missing values with the highest (1) and lowest (0) possible values, respectively. While this method is restricted to binary variables, it offers an estimate of treatment effects for the entire treatment sample, not just “always-reporters”. For that reason, we will use both methods for robustness checks.

Covariates

We will control for some relevant variables in analysis. A list of variables to be considered for inclusion as controls is found [here](https://docs.google.com/a/poverty-action.org/spreadsheets/d/1aSV0kUxZ-sJsRP-yEnMK8uTPVfx8DdqFV9Wfpra6bc0/edit?usp=drive_web).

All models will include the 9 covariates used for stratification and re-randomizations to ensure balance.

For the GLMM model, where we face a constraint on degrees of freedom for the top-level estimation, the selection of additional covariates from this list will be guided by random forest modeling. This methodology: (1) can be used with correlated predictors, (2) requires no formal distributional assumptions, (3) can approximate arbitrary functional relationships between predictor and response variables and allows these relationships to be visualized and interpreted using partial dependence plots, (4) is able to model complex interactions among predictor variables, and (5) assesses the importance of predictor variables by their impact on the accuracy of predictions.

Heterogeneous treatment effects

We are interested in studying whether the effect of CBRLM varied across villagers and regions of different types. For each of the pre-treatment covariates outlined below, we will test for heterogeneous treatment effects through the inclusion of an interaction term between treatment and the covariate. For the purposes of multiple hypothesis correction, we will treat each subgroup over which we estimate impact as a separate hypothesis to be corrected for.

General heterogeneous effects:

**Regions**. The administrative regions covered by the project vary in their community structure (e.g. nomadic pastoralists in the west, absentee commercial farmers in the central regions, sedentary villages in the east) and their level of government support, both of which may have affected the success of the intervention. Additionally, different implementation teams from GOPA worked in each region, and their varying approaches to teaching CBRLM may have had an effect on the observed impact of the intervention.

**Precipitation.** Since the growing conditions for rangeland plants are better under conditions of higher rainfall (described in detail below), we hypothesize that behavioral changes from CBRLM will be greater in areas of higher rainfall as well, as the potential payoff of compliance is larger in those areas.

**Community Ethnic Fractionalization**. A body of literature suggests that collective action and public goods provision is more difficult in ethnically fractionalized communities (Miguel 2005, Easterly and Levine 1997). We hypothesize that the effects of CBRLM on behavioral and social change will be muted in highly fractionalized villages.

Ethnic fractionalization will be measured by an inverted Herfindahl index, which is defined as

1 - Ʃi([proportion of ethnic group i]2)

This value is maximized when there are a large number of small ethnic groups in a village. In this case, ethnicity is proxied by the language that each surveyed household in the village speaks.

**Individual Age and Education**. There is anecdotal evidence that younger, more educated community members were more likely to participate and benefit from CBRLM, in part because the program sought to change long-standing social norms about livestock management and marketing, and in part because some project components (eg managing a grazing area committee) may have been more accessible to educated farmers.

**Individual Gender**. Because the program specifically sought out female kraal managers to participate in CBRLM and serve on grazing area committees, they may have differentially benefitted relative to males in their community.

Rangeland heterogeneous effects:

**Regions**. See discussion above.

**Precipitation.** Conditions of higher rainfall lead to generally better growing conditions for many rangeland plants. If grazing is light to moderate, such plants can respond to the higher precipitation by producing more leaf and root material, and eventually setting seed. If grazing is too heavy and forage tissue removal is too severe, then the ability of the plants to take advantage of the higher rainfall is lessened.

Thus, it is under conditions of higher rainfall where the positive impact of improved grazing management on the grass community is more likely to be observed. This impact would be even more likely to be observed if the soil surface has not been too badly damaged by erosion.

In drier conditions, the more erratic rainfall poses a heightened risk whereby the effects of better management are much less likely to be observed. This is because the plants have fewer opportunities to fully express themselves by adequately growing under a low to moderate grazing regime because they lack sufficient water.

**Soil Type.** Because the eastern portion of the project area is dominated by a generally flat landscape with deep, sandy soils that are very difficult to damage or erode, only minor to moderate rangeland degradation (i.e., reduction in perennial grasses, replacement of perennial grasses with lower-value annual grasses and weedy forbs, and establishment of more woody plants as grass cover is reduced and fire risk lessens) is likely to have occurred in these areas. Improved grazing management could conceivably restore some of these sites unaffected by woody encroachment, for example, by promoting more perennial grass cover relative to annuals because vulnerable perennials can be protected from excessive herbivory.

Because the western portion of the project area is dominated by an often hilly landscape with top soils comprised of more-structured silt, loam, or clay, past overutilization has more often led to severe (and permanent) site damage in the form of gully or rill erosion. Restoring the productivity of such sites is often uneconomic--and impossible simply with changes only in grazing management--because the landscape must first be repaired by filling the gullies and controlling surface water flows. This process would require large inputs of human labor or heavy machinery. Once the landscape is repaired and levelled, then grazing management could have a chance of success.

**Vegetation Type.** Since the biome of an area is shaped by rainfall, soil type and other environmental factors in complex ways, an indicator for vegetation type captures important characteristics about each site that may not be picked up by direct measures of precipitation and soil type. We expect to see greater impacts of the intervention in the savannas and forests of the east compared to the sparser shrublands and grasslands of the central and western regions.

Exploratory analysis

In addition to the primary outcome analysis described above, we will conduct several supplementary analyses in order to give context to the main results. These analyses will not be adjusted for multiple hypothesis correction and will be reported separately from the main results.

**Headman and community project analysis:** IPA conducted interviews with the customary chiefs (aka “headmen”) of each of the 123 grazing areas in the study and gathered information on their demographics and attitudes as well as the community projects and water points in their jurisdiction. It is believed that headman attitudes may have had an effect on the implementation of the project, and may have affected the number of community infrastructure projects, so analyzing these variables will help frame the main results.

The headman survey variables listed in Appendix 3a will be analyzed via a linear regression of treatment status on outcome.

**Direct observation analysis:** In order to verify the self-reported data on herding practices from the 2016 survey, IPA visited a random subset of 370farmers who reported following combined herding and directly observed their herding. Analyzing this data will calibrate the accuracy of the self-reported behavioral outcomes and provide additional descriptive richness.

The grazing direct observation variables listed in Appendix 3b will be analyzed via a linear regression of treatment status on outcome.

Additionally, surveyors visited the community groups (water point committee, grazing group, and village development committee) in each grazing area in 2017 to view their minutes and contribution logs. The data from this activity can help verify our self-reported data on community group participation and financial contribution.

The community group direct observation variables listed in Appendix 3b will be analyzed via a linear regression of treatment status on outcome.

**Fencing analysis:** In the 2016 kraal manager survey and 2017 household survey, we collected data on whether or not respondents fence any of their grazing land and why. Fencing is not straightforwardly “good” or “bad” from a project outcomes prospective, but it may have been affected by the intervention and could play an important role in grazing dynamics.

The fencing variables listed in Appendix 3c will be analyzed via a linear regression of treatment status on outcome.

**Movement analysis:** The CBRLM project may have affected farmers’ decision to move their cattle to a different grazing area in several ways. On one hand, the project may have induced farmers to relocate their herd to a new GA in order to combine with other herds. On the other hand, if the project was successful at preserving grass, farmers in treatment areas may have had less of an inducement to travel in search of greener pastures. Additionally, treatment GAs may have been more successful at keeping farmers from neighboring communities from entering the GA and poaching their grass.

**Intra-kraal cooperation analysis:** Successful implementation of the CBRLM practices may have been mediated by the level of cooperation between the kraal managers and the other households who own cattle in their kraal (who often live far away).

**Political participation analysis:** Since the CBRLM project sought to improve community governance, it may have affected treated individuals’ participation in formal political and governmental institutions. Although this was not a direct goal of the project, it may be reflective of broader shifts in project participants’ attitudes toward institutions.

**Cattle marketing descriptive data:** To provide context for the project’s cattle marketing efforts, we will present descriptive statistics on the prevalence of cattle sales, reasons for selling, reasons why attempted sales were unsuccessful, and most common buyers, broken down by region.

**Declining Treatment Effects**: We hypothesize that treatment effects on behavioural outcomes attenuated in the years following the removal of financial and technical assistance in 2014.

A variety of (most behavioral) outcomes were measured in both 2014 and 2016 / 2017. However, the full indices rarely match perfectly between the two time periods, both because some variables were added in later surveys and the wordings of questions were changed. We will report all outcomes for 2014 and 2016/2017, but explore change over time for those variables measured using the same question in both periods.

**Behavioral Games**: During the kraal manager survey in 2016 and household survey in 2017, we conducted standard four-person public goods games (although each individual was playing alone with three anonymous members of their community and rewards were calculated after the survey).

The behavioral games were played under the following conditions (1) with members inside the community, (2) with individuals from nearby communities, (3) under threat of punishment from other players (once inside, and once outside), and (4) under threat of punishment from the Traditional Authority in the community. The games also elicited individuals’ willingness to pay-to-punish other players if the respondent did not approve of their contribution level.

We hypothesize that CBRLM increases willingness-to-give among farmers in treatment areas and the ratio of willingness-to-give when playing with farmers inside the grazing area relative to farmers from outside.

We also hypothesize that farmers in treatment grazing areas will be more willing to punish (they will on average punish for smaller deviations and on average be more willing to punish at all) both insiders and outsiders, and that Traditional Authorities in treatment grazing areas will be more likely to punish.

**Behavioral / Rangeland Correlation:** In order to better understand how particular herding behaviors (e.g. planned grazing, combined herding, bunched herding) effect particular rangeland health indicators, we will examine the correlation between key behavioral and rangeland outcome variables.

Similarly, in order to understand how differing implementation of the CBRLM principles by GOPA’s regional teams affected rangeland outcomes, we will examine the correlation between the intensity of intervention inputs (index I2 in appendix 1) and rangeland variables.

Neither of these tests are intended as causal analysis, but they will shed light on the mechanisms through which CBRLM may have affected rangeland health.

**Inequality:** We will explore the role of intra-grazing area inequality in cattle wealth both as a pre-treatment covariate that modulates the effect of treatment and as an outcome of interest.

**Monitoring:** Accurate knowledge of other community members’ resource use is a necessary precondition for effective communal management. We asked farmers some questions about nearby respondents’ herding and water use behaviors, and will compare their answers against the data collected in our behavioral survey as well as the observed community group minutes.

[Appendix 1: Outcome Variables](https://docs.google.com/spreadsheets/d/1vwjwiWPiOZO1L94KljxdOMFmzP-szaVqaBPBzecKRzE/edit#gid=597052648)

[Appendix 2: Covariates](https://docs.google.com/spreadsheets/d/10b4ArLC7LyyyBnNfLRlSfgGKXihC7mrpB65YR8ihzvY/edit#gid=0)

[Appendix 3: Exploratory Analyses](https://docs.google.com/spreadsheets/d/1vwjwiWPiOZO1L94KljxdOMFmzP-szaVqaBPBzecKRzE/edit#gid=1970856486)

1. The list of variables includes: **IPA interview data:** community willingness to adopt new programs/technology, traditional authority’s willingness to adopt new programs/technology, community’s social cohesion, worries about grass poaching/spillover, community's rating of herder turnover, cell phone reception; **Census data:** portion of households made of mud/clay/brick, presence of Himba community; **Atlas of Namibia data:** presence of water installation, cattle carrying capacity [↑](#footnote-ref-1)